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IS REGENERATION A REPETITION OF THE ONTOGENETIC AND PHYLOGENETIC PROCESSES?

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FACTS are as unaffected and permanent as the universe. They are always present, whether we know them or not. Theories, on the contrary, are transitory conceptions, products of the intellect striving to comprehend the outer world. As a verbal expression of the unity underlying the overt diversity of phenomena and things, and as a mental picture of the connection between things, theories may be either perceived or imagined, a step towards absolute truth or a profound fallacy. Its fate depends wholly upon compliance with the facts, and it can not, therefore, be used as a *criterion of value* of the facts. Theories may ultimately be revised or discarded —facts remain forever.

With this commonplace in mind, we can clearly see that the question before us—namely, is regeneration a repetition of the processes of ontogeny and phylogeny?—is a matter of theory. As such, it is a much-debated subject which has been so often and so ably discussed in the past by several writers, that I should, perhaps, refrain from adding my quota to the dispute. But it has lately been called to my attention by an admirable monograph, by K. N. Davydoff.[1] As Davydoff writes in Russian, his work is practically inaccessible to American readers.

Considered from a purely technical standpoint, the monograph leaves little more to be desired. It is supplied with a large number of well-finished figures, and the text is lucid and concise. I should use only terms of praise if I were to speak on that score. I shall, however, not concern myself with this side of the matter, as it is my prime object in the present article to scrutinize Davydoff's theoretical position.

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[1] K. N. Davydoff, "Observations on the Process of Regeneration in Enteropneusta," *Mem. de l'Académie Impériale d. Sc. de St.-Pétersbourg*, Vol. 22, No. 10, pp. 1–120, 1908 (Russian).

On the very first pages Davydoff states the main article of his creed: that the fact that new organs, in the process of regeneration, originate from the same layers from which such organs originated embryologically proves that the two processes are parallel in cause. Since the hypothesis of the repetition of the phylogenetic processes in regeneration necessarily rests upon this assumption, as a major premise, it may be well, in the first instance, to examine closely its validity. This plan is preferable also because Davydoff himself in imparting his data and defending his thesis follows a similar course.

The reader is doubtless familiar with the way a comparison of the two-layered gastrula with an adult Cœlenterate had ultimately grown into Haeckel's celebrated "Gastræa-theorie." This broad embryological conception, purporting to bind the entire animal kingdom with bonds of genetic relationship, postulates the homology of adult organs differentiated from similar germ-layers in the embryos. So fascinating was the application of this greatest biological generalization that the overweening confidence it bade became in time a source of grievous blunders. Contradictions have arisen on the ground of striking differences in the origin of organs in development and in budding, as was discovered to be the case in Bryozoa and in Tunicates. Likewise the discrepancy often observed between the methods of organ-formation in ontogeny and regeneration tended further to undermine credulity in the theory and the value of the germ-layers as a criterion of homology. Indeed, so seriously was this aspect of the theory threatened that conservative men, like L. Schultze, anxious to safeguard it from impending disrepute, were obliged to eliminate instances of budding and of regeneration from a consideration of homologies.

> . . . Die Entstehung eines Organs durch Regeneration oder Knospung keinen Anhaltspunkt giebt zur Beurteilung des Morphologischen Wertes seiner Entwicklung aus einem Keimblatt, d. h. die Verwertbarkeit der Keimblatt-Herkunft eines Organs für die Frage seiner Homologie mit einem anderen Organ, dessen Keimblattursprung ebenfalls bekannt ist, wird durch die Knospungs- und Regenerations-befunde in keiner Weise beeinträchtigt. (L. Schultze, p. 331.)

The inextricable contradictions which sprang from the tenets of historical and morphological significance of the germ-layers called forth even a more vigorous reaction. Braem subjected the theory to a thorough and relentless critique revealing its inadequacy. "Der Begriff Keimblatt," he proclaimed, "ist gar kein morphologischer sondern ein physiologischer Begriff. Keimblätter sind Organbildner." And further, he introduced the pragmatic principle that

> . . . Ein Schicht ist nicht deshalb Entoderm weil sie das innere Blatt einer Gastrula ist, sondern sie ist Entoderm weil sie den Darm bildet, weil sie die physiologische Charaktere des Darmblattes entweder bereits besitzt oder doch im Laufe der ferneren Entwicklung annimmt. . . . Massgebend ist nur die organbildende Potenz, die Funktion der zellschicht. (Braem, p. 431.)

While embryologists were thus questioning the importance of the germ-layer as a criterion and in fact were at a loss to know what was meant by a germ-layer, students of the histogenetic processes in regeneration were accumulating evidence to the effect that the new tissues generally arise from corresponding old tissues, and, consequently, from the same germ-layers from which those tissues had been differentiated in development. Although the evidence is based largely upon the investigation of the regenerative process in worms, the facts relating to this animal group as they are stated by various authors are too much at odds with each other to be of any real worth, at least so far as the problem of the relation between regeneration and ontogeny is concerned. This may seem a very strong statement, but it is justified by the frequent contradictions which one encounters in reviewing the literature. To be specific, I might mention the case of *Lumbriculus*. The histogenesis of the regenerated organs in this worm was studied very extensively by at least four investigators. There is scarcely a point of any importance upon which all are unanimously agreed. Unless one shares Davydoff's rosy optimism that "whatever is last is best," and that the latest contribution to the subject is necessarily the truest, the non-committal attitude of skepticism would seem far preferable.

Furthermore, the histological studies of the regenerative process have likewise revealed certain striking deviations from the usual course of things. Wolff's researches of the regeneration of the lens in salamanders showed that in regeneration the lens is formed by a method entirely different from that observed in embryonic development. In the embryo it arises as a thickening of the ectoderm covering the optic cup, while in regeneration—"Der Obere Irisrand ist nämlich offenbar die zweckmässigste stelle für die Enstehung der Linse."

Hazen discovered that in the anemone *Sagartia* the regeneration of the œsophagus involved some important departures from the ontogenetic process.

> In small pieces the œsophagus regenerates as an invagination of mesoglæa and endoderm in the shape of an inverted cup, in which the mesoglæa forms the middle layer, and which is covered on both outside and inside by entoderm. The ectoderm takes no part in the regeneration of the œsophagus.

Margaret Reed found that the muscles of a regenerating appendage in crustacea originate entirely from ectodermal cells. "When the first leg of the crayfish is thrown off at the breaking joint, no muscles are injured, and the muscles for the new leg are formed from cells proliferated by the ectoderm. In the hermit crab also the muscles of the new leg are formed by ectodermal cells." In ontogeny the muscles originate from endoderm.

The last discovery is of particular importance, not only on account of the different methods of muscle-formation in development and regeneration which it brought to light, but because it calls into question the conception of the determined specificity of tissues. And in point of fact, the problem of the relation between regeneration and ontogeny ultimately resolves itself into this wider problem of the specificity of tissues. In the crayfish and hermit crab cells, which under ordinary conditions of development never give rise to muscles, apparently have the potentiality to do so under certain circumstances.

The exceptional cases, alluded to above, are not of the kind that would tend to corroborate the rule. On the contrary, they tend to turn the whole question topsy-turvy. It rests with the adherents of the view that regeneration and ontogeny are parallel processes to bring forth a creditable explanation of these facts to fit them into their theory, for as long as they remain undisputed facts they must likewise remain the unapproachable stronghold of skepticism.

Davydoff in his recent monograph traced with great skill and painstaking care the history of each regenerating organ of *Ptychodera*. This laborious investigation led him to conclude that "the study of regeneration in *Enteropneusta* entirely corroborates the hypothesis that . . . all organs and tissues regenerate from elements of the same germ-layers from which they also developed in ontogeny" (p. 78).

This conclusion is adopted "even though at times there is no *complete* resemblance to the ontogenetic process" (*ibid.*).

The reservation is specially noteworthy since in another place we read the following:

> It must be pointed out that many departures from the general method of regeneration to be described further are of frequent occurrence; in fact, it might be said that each particular case presents some characteristic peculiarity: the details of the regenerative process vary indefinitely. . . . It is necessary, however, to account for all the conditions of each case in order to interpret any deviation from the typical; at present this is an impossible task. Only by making due allowance for

variations within the widest limits, it may be possible to delineate the normal course or type of regeneration (p. 16).

Without any intention on my part to detract from Davydoff's merits of having accurately ascertained the facts of the regeneration of *Enteropneusta*, it nevertheless seems to me that the manner of his argument is more entertaining than convincing.

Here, for instance, are samples of his reasoning. In *Ptychodera* (*Enteropneusta*) the new cœlome regenerates from elements of the old cœlome, *i. e.*, the new mesoderm arises from old mesoderm. So far, so good. Davydoff, however, is not fully satisfied with this achievement, as he is determined to prove to his reader that the parallelism between ontogeny and regeneration is of a far more subtle nature; but he encounters a stumbling block in that the cœlome in ontogeny arises as an evagination of the primitive endoderm. In the regeneration of the cœlome, however, the endoderm takes no direct part. Is that to be regarded as a discrepancy between ontogeny and regeneration? Sensible men might say that since the old cœlome had been differentiated from endoderm, therefore, the new cœlome regenerating from the old cœlome is likewise of endodermal origin, and would let the matter rest there. Davydoff is not contented with this sort of proof. He finds that in *Balanoglossus kowalewskii* the cœlome of the collar arises not as an independent outgrowth from the primitive gut, but is formed by proliferation of cells from the cœlomic pouches of the body. In other words, in *Balanoglossus kowalewskii* the cœlome of the collar originates not directly from the endoderm, but indirectly from the already differentiated cœlome of the body. Is not this a wonderful identity between the processes of regeneration and ontogeny in *Ptychodera* and *Balanoglossus?*

But how is it with the cœlome of the proboscis? In *Ptychodera* it regenerates by proliferation of cells from the cœlome of the collar. Does it arise in the same manner in the course of ontogeny? Davydoff appears a little embarrassed on this score. *Balanoglossus kowalewskii*, which served him such a good turn in his contention regarding the subtle similarity between the regeneration and development of the cœlome of the collar, is of no avail for the present purpose. In *Balanoglossus kowalewskii* the cœlome of the proboscis arises immediately from the primitive gut as an unpaired outgrowth. But what does it really matter how this end is accomplished in *Balanoglossus kowalewskii?* And why, proceeds Davydoff, should we be hasty in emphasizing too strongly this difference between ontogeny and regeneration, as long as we know absolutely nothing concerning the formation of the cœlome of the proboscis in the ontogeny of *Enteropneusta?*

Davydoff's criticisms, after the fashion of his argumentation, is superficial and unsubstantial. His criticism of Morgan misses the point completely. Attacking Morgan as the foremost leader of the opposite camp, and as one who gave the ablest expression to the skepticism concerning the supposed causal relation between processes of regeneration and ontogeny, Davydoff remarks:

> We must agree that Morgan's arguments as well as his whole critique are exceedingly weak. While referring in the literary index [Regeneration, 1907] to a number of works contradicting his views, he none the less makes no mention of them in the text, but exalts a few facts, which are in reality of no great significance, and often even count directly against Morgan's own contentions (p. 65).

This statement requires some comment, so unjustified does it appear to me. In the first place, that Morgan refers to works which take the other side in the disputed question is a good warrant that the question has not been considered one-sidedly. Davydoff, on the contrary, for some esoteric reasons and without any excuse whatever, closes his eyes upon the facts which challenge his hypothesis. Furthermore, Davydoff's allusion to the work of Abel, as one of a number of works supporting his assumptions, might likewise be questioned upon Abel's own authority. For Abel, enumerating the different ways of the regeneration of the end-gut, states definitely:

> Hinsichtlich des Verlaufs der Regeneration braucht jedoch durchaus keine mit den ontogenetischen Processen übereinstimende Bildungsweise des Enddarmes stattzufinden.

In the second place, Davydoff's criticism is unjust because it arises from a lack of appreciation of the soundness of Morgan's skepticism. As a matter of fact, Morgan and his school do not *deny* that there is at times a very marked similarity between the methods of regeneration and ontogeny. Morgan is willing to go even further than that, and to admit a complete identity of the two processes. His position may be best formulated in his own apt words:

> The mistake, I think, is not in stating that the two processes are sometimes similar, or even identical, but *in stating the matter as though the regenerative process repeats the embryonic method of development*[2] (p. 213).

I dwell at such great length upon the question of the causal relation between regenerative and ontogenetic processes because, unless there is reasonable occasion for disbelieving it, the theory of the repetition of phylogenetic processes in the course of regeneration must not be dodged, as the next logical step. In the foregoing I have been emphasizing particularly the facts opposed to the theory not from any prejudiced neglect of the facts apparently favorable to the theory, but simply to show that the question of the connection between ontogeny and regeneration is far from being firmly established. To build upon this as a foundation elaborate theoretic superstructures is like erecting a magnificent edifice upon a foundation of quicksand.

But if regeneration repeats the ontogenetic process, as is claimed by some, what is the significance of the occasional departures of the end result of regeneration from that of normal development? The believers in the theory have a ready answer. Those are obviously cases of atavism, they say. Just as the connection between ontogeny and phylogeny is obscured by the cœnogenetic peculiarities of the former, so likewise the connection between ontogeny and regeneration is frequently obscured by the palingenetic peculiarities of the latter. To quote: "Die Uebereinstimmung (zwischen Regeneration und Embryonalentwicklung) bezieht sich gerade auf palingenetische züge, wärend cenogenetische viel leichter eliminiert werden" (E. Schultz). The application of the principle of atavism to phenomena of regeneration has been in vogue for nearly a quarter of a century, and Davydoff also records what he considers cases of atavism in the regeneration of Ptychodera, such as: the formation of two nephridial ducts instead of one; the failure to regenerate on the part of the ectodermal duct of the nephridium; the regeneration of a double instead of a single pericardial sac, etc.

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[2] Italics are mine.

The novelty of Davydoff's work consists in the attempt to give to the principle of atavism practical application. If regeneration proceeds by more primitive methods, why not avail oneself of this opportunity to throw light upon such points of animal morphology as are obscured by the cœnogenetic methods of development? To Davydoff belongs the full credit for having ventured to accept this logical consequence of the preceding propositions. We shall return to this question later. At this moment I wish to examine the validity of the so-called "atavism" theory in regeneration.

The defects of this theory are twofold—the theory is not self-consistent, and, moreover, in the majority of cases it takes for granted what ought first to be proved. The inducement may be strong to interpret, for instance, the type of scaling on regenerating tails of lizards, totally different from the normal for the given genus and yet similar to that of another genus, as a "throwing back" to some common ancestral condition. The appearance of dorsal stripes on the regenerating tails instead of the usual annuli might with like justice be conceived as a "throwing back." But while maintaining the atavistic nature of the former, Boulenger finds no occasion for applying the theory to the latter case.

Barfurth finds that the axolotl often regenerates five fingers on an amputated limb. Considering the complexity of the regenerative mechanism, the frequent occurrence of abnormalities, and, as Barfurth himself points out, that "Modus und Product der Regeneration von der Art der Operation abhängig sind" it would seem that the regeneration of five fingers is of no more value from the point of view of the salamander's phylogeny than the double paw of a cat is for the appreciation of its phylogeny. Yet he is ready to believe that "die verhältnismässig häufige Regeneration einer fünffingerigen Hand beim Axolotl ist ein Rückschlag auf die ursprünglich normalerweise fünffingerige Hand der Amphibien."

It will, perhaps, not be devoid of interest to note that Ridewood, who studied the regeneration of the limb of the toad, expresses himself quite differently upon this question. "While in animals other than Anura," he says, "structural differences between the regenerated and the normal limb may be explained as phenomena of atavism, there is no evidence of such phylogenetic reversion in the regenerated limb-skeleton of the Anura under consideration."

It should be obvious that it is not sufficient to point to some abstract ancestor, an imaginary conception, but that the real ancestor must be known in order that the genuineness of the reversion may be established beyond doubt. It is absurd to apply the term "reversion" and "atavism" to sporadic growths, not represented in the normal development, of which the factors are, in most cases, entirely unknown to us.

The inconsistency ascribed to the theory may be further exemplified by the following instances. In tracheate insects, according to Brindley, the reproduced portion of an amputated appendage is invariably unlike the normal. In Blattidæ fewer than five tarsi regenerate, and the size of the parts is likewise different from the normal. Is the reduced number of tarsal joints in the regenerated appendages an indication of reversion to a more primitive condition? No advocate of the atavism theory would be likely to go so far as to oppose the authoritative opinion of entomologists which assumes the five-jointed condition as the primitive one. Yet even on its full face value this fact of dropping out of joints is in no sense different from the fact of the addition of an extra finger in the regenerating limb of a salamander which is professed to be a reversion. And, to be consistent, should we not also suppose on the basis of Herbst's discovery of the regeneration in crustacea of antennæ in place of extirpated eyes that the ancestors of these animals at the dawn of their history had no eyes, and that the antenna is the precursor of the eye?

One of the most amusing attempts to mould a regenerated structural peculiarity into the picturesque shape of an ancestral structure of overwhelming antiquity is that of Schimkewitsch, who very sagaciously interprets the regeneration of an amphibian lens in terms of atavism. As already mentioned in a previous section of the paper, the amphibian lens regenerates not from the skin but from the iris, which is a marked departure from the developmental process. This fact leads Schimkewitsch to suppose that the paired eyes of vertebrates must have primitively been similar to the epiphysis of reptiles, commonly known as the pineal organ or eye. This pineal organ possesses a lens-like structure which is formed by the thickening of the outer wall of the cup. The purely superficial resemblance of the lens regenerating from the iris of the eye and the lens-like thickening of the pineal organ is the starting point of Schimkewitsch's hypothesis. He says:

> Ursprünglich die paarigen Augen eine ebensolche Linse besassen wie wir sie im unpaaren Augen der Hatteria sehen, d. h. hervorgegangen aus der Wand der Augenblasse selbst.

It is needless to insist that, in the usual fashion, the author of the hypothesis is appealing for proof to a fantastic ancestral condition. But quite apart from that, the looseness of his argumentation is further increased by the circumstance that anatomists are by no means certain that the pineal body is an eye, and judging by the structure of its lens it may also be a heat-perceiving organ, as some indeed have suggested.

Thus far I have been considering what might properly be called the preliminary steps leading to the hypothesis, that the end result of regeneration differing from the usual result of ontogenetic development lends the key to a solution of obscure morphologic problems. This suggestion or idea is at the bottom of Davydoff's entire work; it persistently crops up here and there throughout the monograph. In short, it is the soul of that work.

From what has been said in the foregoing it ought to be clear that if the hypothesis of the repetition of ontogeny in regeneration, and also that of the atavistic nature of the deviations from the normal condition, are not false assumptions, they are at least deserving the verdict "not proven." Since in no known system of logic does the truth issue from propositions, either wholly untrue or else of such uncertain veracity as to leave free choice to intellectual likes and dislikes, it would seem that Davydoff was laboring largely under a mistaken principle.

I am, however, ready to go to the extent of granting, just for the sake of further argument, that the first two propositions are demonstrably true, and that consequently Davydoff's idea of using regenerated peculiarities for the purpose of solving obscure problems of phylogenetic importance is, humanly speaking, beyond objection. A moment's consideration will not fail to convince us that even though truth may be a quality of Davydoff's principle, its function could be either of purely negative value or even of no value.

The literature on regeneration abounds in instances of departures from the normal which appear either sporadically or even regularly in the course of regeneration of organs. Earthworms regenerate heteromorphic tails and planarians regenerate heteromorphic heads. Do these peculiarities signify any reversion to a more primitive condition? Double and multiple structures likewise frequently appear in the regeneration of organs, as, for instance, the regeneration of double heads in planarians or in *Lumbriculus.* Do these abnormalities bring any message about the normal state of things in the remote past? Likewise, as Davydoff informs us, *Enteropneusta* may regenerate double pericardial sacs or double notochords. What is the morphological and historical significance of these unusual formations? Going through a list of such irregularities and departures from the normal, one may doubtless find all gradations from the obvious freaks of nature down to such as may claim a respectable "atavistic" distinction. But the question is, what will guide us in discriminating between these various departures, for after all every departure in regeneration originates under the special conditions of an amputation. The manner in which Davydoff overcomes the difficulty of this question is both characteristic and interesting, and we shall return to it soon. It should be pointed out, however, first that no existing theory of phylogenetic significance can be invalidated to the slightest degree by the failure to regenerate on the part of such structural peculiarities or characteristics as may be postulated by the theory as primitive or ancestral. On the other hand, any sporadic growth in the process of regeneration, if by play of chance it should happen to coincide with a theoretical anticipation, would become an additional support to the theory. In other words, in any disputed theoretical problem of animal phylogeny the evidence derived from the study of regeneration must by the limitations of its own nature be always of the affirmative kind. It is the better part of wisdom to believe much too little rather than a little too much, and on this account we should hesitate to rely upon evidence from regeneration.

But to return to the original question as to what should guide us in deciding where, in regeneration, mere abnormalities or monstrosities end and where ''palingenesis'' and ''atavisms'' begin. Let us see how Davydoff proceeds with this question. He records among others two cases where occasionally double structures regenerate instead of the normally single structure, viz., the doubling of the pericardial sac and of the notochord. The meaning of these parallel facts is equally puzzling to us and, broadly considered, the two facts are to all intents and purposes of the same importance. But Davydoff rummages through the volumes in the library for light upon the significance of these extraordinary instances. There he discovers that the much-esteemed, and certainly authoritative writer of the ''Beiträge zur einer Trophocœlthorie'' in speaking of the ''Herzblase'' of *Balanoglossus* suggests in parentheses and under the auspices of a question mark that the ''Herzblase'' may have been primitively a double structure. This suggestion is expressed just in two words—(ursprünglich paarige?). Evidently Davydoff had not succeeded in finding in the literature another similar hypothesis that the notochord, too, may have been primitively a double structure. I judge so, because without offering any further reasons, he dismisses the puzzle of a double notochord by proclaiming it an abnormality, while honoring the double pericardial sac with the distinction of atavism.

Thus does Davydoff solve the question, and whatever merits or demerits his answer may have from the point of view of common sense, one thing is certain: that there is no direct, immediate way of deciding the question. We must fall back upon a theory, a hypothesis, a suggestion, even a mere guess, in distinguishing between what is abnormal and what is ancestral in regenerated structures. In a word, it is the theory which, in accordance with Davydoff's solution of the question, must give value to the observed facts.

This *principle* is diametrically opposite to the principle laid down in the introduction to this paper, that the nature of the relation between facts and theories is such that the facts can not be valued by their compliance with theories. We need theories for inspiration and enlightenment, we need facts to build upon; but to discriminate between facts in favor of any mental conception is to place oneself upon the inclined plane of immoderate speculation.

To sum up: While the evidence shows that as a rule organs originate from similar germ-layers, both in ontogeny and in regeneration, there are also some striking exceptions to the rule. The hypothesis, that the method of regeneration is causally influenced by the course of ontogeny, is, therefore, quite unnecessary as a corollary. With the elimination of this hypothesis the conception of the atavistic nature of regenerated peculiarities, *i. e.*, the conception of a repetition in regeneration of phylogenetic processes, loses its chief logical support. This last theory, however, is also objectionable (1) because of its inherent inconsistency, (2) because it depends upon more or less problematic assumptions.

With both hypotheses, those of the repetition in regeneration of ontogenetic and of phylogenetic processes, now discredited, it would be venturesome to take sides in unsettled questions of animal morphology upon the ground of evidence deduced from a study of regeneration. But even if the hypothesis were correct, to accept it as a working principle is to put oneself deliberately into the logician's vicious circle—proving theories with facts approved of by the theories.

BIBLIOGRAPHY

Abel, M. Regenerationsvorgänge bei den limikolen Oligochaeten. *Zeitschr. f. wissenschaftl. Zool.*, Vol. 73, pp. 1–74, 1902.

Barfurth, D. Die experimentelle Regeneration überschüssiger Gliedmassentheile (Polydactylie) bei den Amphibien. *Arch. f. Entwicklungsm.*, Vol. 1, pp. 91–116, 1894.

Boulenger, G. A. (*a*) On the Scaling of the Reproduced Tail in Lizards. *Proc. Zool. Soc. London*, p. 351, 1888. (*b*) On an Iguana with Reproduced Tail. *Proc. Zool. Soc. London*, p. 466, 1891.

Braem, F. von. Was ist ein Keimblatt? *Biol. Centralbl.*, Vol. 15, pp. 427–443; 466–476; 491–506; 1905.

Hazen, H. P. Regeneration of the Œsophagus in the Anemone *Sagartia luciæ*. *Arch. f. Entw.*, Vol. 14, pp. 592–599, 1902.

Hepke, Paul. Ueber histo- und organo-genetische Vorgänge bei den Regenerationsprocessen der Naiden. *Zeitschr. f. wissenschaftl. Zool.*, Vol. 63, pp. 263–291, 1897.

Brindley, H. H. On Certain Characters of Reproduced Appendages in Arthropoda, particularly in the Blattidæ. *Proc. Zool. Soc. London*, pp. 924–958, 1898.

Davydoff, K. N. Observations on the Process of Regeneration in Enteropneusta. *Mem. de l'Académie Impériale des Sciences de St.-Pétersbourg*, Vol. 22, No. 10, pp. 1–120, 1908 (Russian).

Morgan, T. H. Regeneration, 1901.

Reed, M. The Regeneration of the First Leg of the Cray-fish. *Arch. f. Entwicklungsm.*, Vol. 18, pp. 307–317, 1904.

Ridewood, W. G. On the Skeleton of Regenerated Limbs of the Midwife-toad (*Alytes obstetricans*). *Proc. Zool. Soc. London*, pp. 101–106, 1897.

Schimkewitsch, W. Ueber den Atavistischen Charakter der Linsen regeneration bei Amphibien. *Anat. Anz.*, Vol. 21, No. 2, pp. 48–50, 1902.

Schultze, L. S. Die Regeneration des Ganglions von *Ciona intestinalis* L. und über das Verhältniss der Regeneration und Knospung zur Keimblätterlehre. *Jenaische Zeitschr. f. Naturwiss.*, Vol. 33, pp. 263–344, 1900.

Schultz, E. Ueber das Verhältniss der Regeneration zur Embryonalentwicklung und Knospung. *Biol. Centralbl.*, Vol. 22, pp. 360–368, 1902.

Wagner, F. von. Einige Bemerkungen über das Verhältniss von Ontogenie und Regeneration. *Biol. Centralbl.*, Vol. 13, pp. 287–296, 1893.

Wolff, G. Entwickelungsphysiologische studien I. Die Regeneration des Urodelenlinse. *Arch. f. Entwicklungsm.*, Vol. 1, pp. 380–390, 1895.